PHILOSOPHICAL

TRANSACTIONS.

I. *The* Bakerian *Lecture. Observations on the Quantity of horizontal Refraction; with a Method of measuring the Dip at Sea. By* William Hyde Wollaston, *M. D. F. R. S.*

Read November 11, 1802.

In a Paper which I some time since presented to this Society, (printed in the Phil. Trans. for 1800,) I endeavoured to ascertain the causes, and to explain the various cases, of horizontal refraction, which I had either observed myself, or had seen described by others.

At the time of writing that essay, I had not met with the *Mémoires sur l'Egypte*, published but a short time before; and I was not aware that an account had been given by M. Monge, of the phenomenon known to the French by the name of *mirage*, which their army had daily opportunities of seeing, in their march through the deserts of Egypt.

In the perusal of this memoir, I could not fail to derive instruction from the information it contained; but, as the facts related by him accord entirely with the theory that I had advanced, I was by no means induced to adopt the explanation that he has proposed, in preference to my own.

The definite reflecting surface which he supposes to take place between two strata of air of different density, is by no means consistent with that continued ascent of rarefied air which he himself admits; and the explanation founded on this hypothesis will not apply to other cases, which may all be satisfactorily accounted for, upon the supposition of a gradual change of density, and successive curvature of the rays of light by refraction.

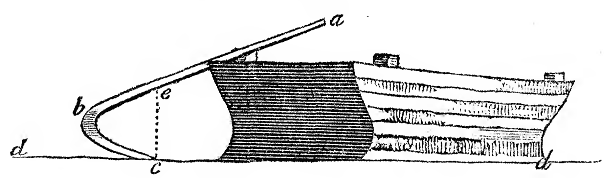
I have since learned that the same subject had also been ably treated by Mr. WOLTMAN, in GILBERT'S *Annalen der Physik;* but I have to regret that his dissertation, as well as that of GRUBER, in the same Annals, were written in a language that was unknown to me, and that I could not avail myself of the assistance that I might otherwise have received from their researches.

When I formerly engaged in this inquiry, being impressed with the advantage to be derived from it to nautical astronomy, on account of the variations in the dip of the apparent horizon, from which all observations of altitude at sea must necessarily be taken, I suggested the expediency of a series of observations, to be made by a person attentive to those changes of temperature or moisture of the atmosphere, on which he might find the depression of his horizon principally to depend. I had at that time no expectation that I could myself pursue this subject farther to any useful purpose, having little prospect of residing for a sufficient length of time in view of the sea, and seeing no other method by which the same end might be accomplished. I have, however, since that time, found means to satisfy myself, by observations over the surface of the Thames, that although the quantity of refraction varies in general with any change of

the thermometer or hygrometer, yet the law of these variations is not altogether so simple as I had hoped it might be found.

I shall, on the present occasion, first relate the facts on which this opinion is founded, and which are in themselves sufficiently remarkable, on account of the unexpected quantity of refraction observable over a short extent of water; I shall, in the next place, shew that the exact determination of the concurrent changes of the atmosphere are of less value, and their irregularities of less consequence, than I had conceived, as there is a very easy method whereby the quantity of dip at sea may be at any time correctly measured; and therefore the end which I sought by indirect means, may be at once directly attained.

The first instance that occurred to me, of observable refraction over the surface of the Thames, was wholly accidental. I was sitting in a boat near Chelsea, in such a position that my eye was elevated about half a yard from the surface of the water, and had a view over its surface, that probably somewhat exceeded a mile in length, when I remarked that the oars of several barges at a distance, that were then coming up with the tide, appeared bent in various degrees, according to their distance from me. The most distant appeared nearly in the form



here represented; *dd* being my visible horizon by apparent curvature of the water; *ab* the oar itself in its inclined position; and *bc* an inverted image of the portion *be*. By a little attention to other boats, and to buildings on shore, I could discern that the

appearance of all distant objects seen near the surface of the water was affected in a similar manner, but that scarcely any of them afforded images so perfectly distinct as the oblique line of an oar dipped in the water.

A person present at the time (as well as some others to whom I have since related the circumstance) was inclined to attribute the appearance to reflection from the surface of the water; but, by a moderate share of attention, a very evident difference may be discovered between the inversion occasioned by reflection, and that which is caused by atmospherical refraction. In cases of reflection, the angles between the object and image are sharp, the line of contact between them straight and well defined, but the lower part of the image indefinite and confused, by means of any slight undulation of the water. But, when the images are caused by refraction, the confines of the object and its inverted image are rounded and indistinct, and the lower edge of the image is terminated by a straight line at the surface of the water. In addition to these marks of difference, there is another circumstance which, if attended to, must at once remove all doubt; for, by bringing the line of sight near to the surface of the water, boats and other small objects are found to be completely hidden by an apparent horizon, which, in so short a distance, cannot be owing to any real curvature of the water, and can arise solely from the bending of the rays by refraction.

When I reflected upon the causes which were probably instrumental in the production of these phenomena, they appeared referrible to difference of temperature alone. After a succession of weather so hot that the thermometer, during one month preceding, had been 12 times above 80°, and on an average of

the month at 68°, the evening of that day (August 22, 1800) was unusually cold, the thermometer being 55°. The water might be supposed to retain the temperature it had acquired during a few weeks preceding, and, by warming the stratum of air immediately contiguous to it, might cause a diminution of its refractive density, sufficient to effect this inverted curvature of the rays of light, in the manner formerly explained. As I was at that time unprovided with instruments of any kind, I had it not in my power to estimate the quantity of refraction, or temperatures; and can only say that, to my hand, the water felt in an uncommon degree warmer than the air.

Being thus furnished with an unexpected field for observation, I from that time took such opportunities as similar changes of the weather afforded me, of examining and measuring the quantities of refraction that might be discovered by the same means over another part of the river, that I found most suited to my convenience.

The situation from which the greater part of my observations were made, was at the SE corner of Somerset house. The view from this spot extends under Blackfriars bridge, towards London bridge, upwards of a mile in length, and in the opposite direction through Westminster bridge, which is three quarters of a mile distant.

Such distances are however by no means necessary; and indeed the air over the river, in cold weather, is generally, or at least very frequently, not sufficiently clear for seeing distinctly to so great distances. For, since the winds which are most likely to effect a sufficient change of temperature, on account of their coldness, are usually from the E, or NE, the principal smoke of the town is then brought in that direction, and hovers, like a dense fog

over the course of the river. This circumstance deprived me of many opportunities which the changes of the thermometer indicated to be favourable for my purpose, and obliged me often to make use of shorter distances than I should otherwise have chosen, by bringing the line of sight as near as I could to the surface of the water.

For this purpose, I had a plane reflector fitted to the object-end of a small pocket telescope, at an angle of 45°, so that, when the telescope was held vertically, it gave a horizontal view at any level that was found most eligible. When the water has been calm, I have observed that the greatest refraction was visible within an inch or two of its surface, and I have then seen a refraction of six or seven minutes in the space of 300 or 400 yards: at other times, I have found it greatest at the height of a foot or two; but, in this case, a far more extensive view becomes necessary.

The first measures that I took were on the 23d of September, 1800. The water was $2\frac{1}{2}°$ warmer than the air, and I found a refraction of about 4′.

Oct. 17. The difference of temperature was 3°, and the refraction 3′.

Oct. 22. The water was $11\frac{1}{2}°$ warmer than the air, yet the quantity of refraction did not exceed 3′.

The smallness of the quantity of refraction upon this occasion, I attributed to the dryness of the atmosphere, conjecturing that a rapid evaporation might in great measure counteract that warmth which the water would otherwise have communicated to the air.

From that time, therefore, I have noted not only the heights of the thermometer in the water and in the air, but have added

also the degrees of cold produced by keeping the bulb of it moistened for a sufficient time to render it stationary. In confirmation of my conjecture respecting the dryness of Oct. 22, I have also, in the following Table, which comprises the whole of my observations, inserted a column from the Register kept at the apartments of the Royal Society, containing the heights of the hygrometer, on those mornings when my observations were made.

TABLE.

At 8, A. M.	Air.	Water.	Difference.	Refraction.	Cold by evaporation.	Hygrometer.
1800. Sept. 23	57	$60\frac{1}{2}°$	$3\frac{1}{2}°$	4′	– –	72°
Oct. 17	$46\frac{1}{2}$	$49\frac{1}{2}$	3	3	– –	72
22	38	$49\frac{1}{2}$	$11\frac{1}{2}$	3	– –	67
Nov. 1	41	$45\frac{1}{2}$	$4\frac{1}{2}$	8	$\frac{1}{2}°$	76
4	$43\frac{3}{4}$	$46\frac{3}{4}$	3	3 –	$1\frac{3}{4}$	72
5	37	45	8	8 +	1	69
12	$44\frac{1}{2}$	$48\frac{1}{2}$	4	1 +	$3\frac{1}{2}$	73
13	40	$44\frac{1}{2}$	$4\frac{1}{2}$	5	$\frac{1}{2}$	76
1801. June 13	50	63	13	9 +	5	65
22	55	61	6	6 +	6	65
23	55	62	7	6	$4\frac{1}{2}$	65
24	55	61	6	5	3	67
Sept. 8	60	64	4	7	2	78
9	64	$64\frac{3}{4}$	$\frac{3}{4}$	5	3	74
10	58	64	6	7	2	70
12 o'clock, 10	63	64	1	2		

From a review of the preceding Table it will be found, upon the whole, that when the water is warmer than the air, some increase of depression of the horizon may be expected; but

that its quantity will be greatly influenced, and in general diminished, by dryness of the atmosphere.

It appears, however, that no observable regularity is deducible from the measures above given; but that the quantity, on some occasions, is far different from what the states of the thermometer and hygrometer would indicate. On the 9th of September, for instance, the difference of temperature is only $\frac{3}{4}^{\circ}$, and the evaporation, to counteract this slight excess of warmth, produced as much as 3° of cold; nevertheless, the refraction visible was full $5'$. In this observation I think that I could not be mistaken, as the water was at the time perfectly calm, the air uncommonly clear, and I had leisure to pay particular attention to so unforeseen an occurrence.

This one instance appears conformable to the opinion entertained by Mr. HUDDART, and by M. MONGE, that, under some circumstances, the solution of water in the atmosphere causes a decrease in its refractive power; but, on no other occasion have I been induced to draw a similar inference.

The object that I have at all times chosen, as shewing best the quantity of refraction, has been either an oar dipped in the water at the greatest discernible distance, or some other line equally inclined; and the angle measured has been, from the point where the inverted image is terminated by the water, to that part of the oar itself which appears to be directly above it. (The apparent magnitude of *ec*, Fig. p. 3.)

The eight first angles were taken with a mother-of-pearl micrometer in the principal focus of my telescope, and are not so much to be depended upon for accuracy as the succeeding eight. These last were measured with a divided eye-glass

micrometer, and consequently are not liable to any error from unsteadiness of the instrument or object.

From the foregoing observations we learn, that the quantity of refraction over the surface of water may be very considerable, where the land is near enough to influence the temperature of the air. At sea, however, so great differences of temperature cannot be expected; and the increase of dip caused by this variation of horizontal refraction, it is to be presumed, is not so great as in the confined course of a river; but, if we consider that it may also be subject to an equal diminution from an opposite cause, and that the horizon may even become apparently elevated, there can be no question that the error in nautical observations, arising from a supposition that it is invariably according to the height of the observer, stands in need of correction.

The remedy employed by Mr. HUDDART,* of taking two angles of the sun from opposite points of the horizon at the same time, and considering the excess of their sum above 180° as double the dip, must without doubt be effectual; but, from causes which he assigns, it is practicable only within certain limits of zenith distance; for, where the zenith distance is small, and the changes of azimuth rapid, there is required considerable dexterity and steadiness of a single observer who attempts to turn in due time, from one observation to another; and, when it exceeds 30°, the greater angle cannot be measured with a sextant, and consequently his method is, with that instrument, of use only in low latitudes.

On account of the difficulty attending some of the adjustments for the back observation, he rejects that method for

* Phil. Trans. for 1797, p. 40.

taking angles in general, with much reason; but he has thereby overlooked a means of determining the dip, which I am inclined to think might be employed with advantage in all latitudes, without any occasion to hurry the most inexperienced or cautious observer.

By the back observation, the whole vertical angle between any two opposite points of the horizon may be measured at once, either before or after taking an altitude. Half the excess of this angle above 180°, should of course be the dip required.

But, if it be doubtful whether the instrument is duly adjusted, a second observation becomes necessary. The instrument must be reversed, and, if the apparent deficiency of the opposite angle from 180° be not equal to the excess before obtained, the index error may then be corrected accordingly; and, since the want of adjustment, either of the glasses at right angles to the plane of the instrument, or of the line of sight parallel to it, will affect both the larger and smaller angle very nearly in an equal degree, the $\frac{1}{4}$ part of their difference will be extremely near the truth, and the errors arising from want of those adjustments may with safety be neglected.

This method of correcting the index error for the back observation at sea, was many years since recommended by Mr. LUDLAM;* yet I do not find that it has been noticed by subsequent writers on that subject, or suggested by any one for determining the dip; but I can discover no reason for which it could be rejected as fallacious, and I should hope that in practice it would be found convenient, since in theory it appears to be effectual.

The most obvious objection to this, as well as to Mr. HUDDART'S

* Directions for the Use of HADLEY'S Quadrant, 1771. § 82, p. 56.

method, is the possibility that the refraction may be in some measure different in opposite points of the horizon at the same time. When land is at no great distance, such an inequality may be found to occur; but, upon the surface of the ocean in general, any partial variations of temperature can rarely be supposed to exist; and it is probable, that under any circumstances, the difference will not bear any considerable proportion to the whole refraction; nor can it be thought a sufficient reason for rejecting one correction proposed, that there may yet remain other smaller errors, to which all methods are equally liable, but which it is not the object of the present dissertation to rectify.